THURSDAY, DECEMBER 22, 1898.

GEGENBAUR'S COMPARATIVE ANATOMY OF THE VERTEBRATA.

Vergleichende Anatomie der Wirbelthiere: mit Berücksichtigung der Wirbellosen. Von Carl Gegenbaur. Erster Band. Einleitung, Integument, Skeletsystem, Muskelsystem, Nervensystem und Sinnesorgane. Mit 619 Zum. Theil farbigen Figuren im Text. Pp. xiv + 978. (Leipzig: Wilhelm Engelmann, 1898.)

THE first volume of the long-expected work by the master has appeared. By placing the study of anatomy upon the basis of evolution he had become the founder of modern comparative anatomy, and he has raised the building to a great extent by his own hands, supported by a school of disciples, ever increasing through his stimulating and correcting influence.

No wonder that for years expectation has been keen about this book, which must necessarily be the crowning of his life's work. The present volume, besides an introduction, deals with the tegumentary, skeletal, muscular and nervous systems, and the sense organs.

It would be a hopeless attempt here to give anything like an adequate *résumé* of the nearly 1000 pages of this first volume. Only some of the salient features can be touched upon. The plan is grand; the execution can be fully appreciated only by those who have made comparative anatomy their special study, because the treatment frequently soars to such heights that the enormous amount of detail which is marshalled in the book, almost seems to disappear before the generalising ideas into which the facts have been welded and condensed. This is especially the case with the introductory chapters prefacing, or the summarising retrospects following upon, the principal chapters. But this does not make easy reading, and some of the sentences, although containing beautifully conceived ideas, are so idiomatic and so terse as to appear almost oracular. For instance, he discusses the development of the bird's wing, how the quills growing upon the patagium, and gaining preponderance over it, have become the functional wing surface, while the patagium itself loses its importance and becomes correspondingly reduced. The result of this contemplation is summed up thus: "Das Product tritt functionell an die Stelle des Bodens, auf dem es entstand."

The general introduction alone, taking up the first seventy pages, is an ideal treatise of morphology, dealing with such fundamental questions as adaptation, correlation, differentiation, inheritance, ontogeny with reference to phylogeny, value and meaning of the germinal layers, &c.

A characteristic feature, going like a red thread through the whole book, is the animosity against ontogenetic research so far as that is in the hands of those whom our author speaks of as embryographers. According to him the ontogenetic record is of no use unless it is in full concord with the results arrived at by the comparative anatomical method, and not many opportunities are missed which afford a stab at ontogeny where this has failed to elucidate a certain problem. The embryographer will, therefore, feel inclined to smile when he comes across the not unfrequent passages where, palæontology remaining mute and comparative anatomy revealing nothing, the problem in question is summarily dismissed as one fit for the ontogenetic method.

Another leading feature is the striving to derive any given organ from something else, from another one previously existing, instead of being satisfied with its origin *in situ*. Of course this is the true scientific, evolutionary method, and it is the very one by which Gegenbaur's works have become epoch-making; but occasionally the idea seems to be a little overdone, and it is then not always easy to reconcile the various arguments with each other. For instance, in the discussion of great importance, pp. 590-592, which runs approximately as follows:

It is more reasonable to derive cartilaginous parts from other existing cartilage, although their ontogeny may show them to make their appearance where they are wanted in the organism. Thus it is, for instance, preferable to assume the derivation of the Cyclostomes' gill-basket from the cranial cartilage, instead of believing that this basket-work has originated *in situ*. "It is (p. 590) very probable that the whole of the cartilaginous skeleton took its origin from the perichordal cartilage."

All this is certainly very satisfactory and uniform, but as the author himself has pointed out on p. 200, it is becoming more and more plausible that the original home of cartilage was in the integument, in the ectoderm. Its appearance in the perichordal neighbourhood would in this case be a secondary feature, owing to chondroid infiltration into the connective tissue. But if this is so, then there would be nothing unreasonable in the assumption that the gill-basket of the Lamprey had received its cartilage *in situ*. We do not want to press this derivation, but it seems unfair categorically to ask (p. 591):

> "What business has a cell, or even a group of cells, to transform itself here or there into cartilage? One or a few cells, even when they become cartilaginous, do not yet possess a supporting function; at any rate the causal momentum of this transformation would remain obscure, as the result of the transformation cannot at the same time be its cause."

This certainly sounds very uncompromising, but the mystery of the origin of the cartilage is not solved by deriving it from other cartilage. The same consideration applies to the derivation of bone, and our author makes the following statement, p. 594:

> "Indem wir das knöcherne Skelet nicht mehr ausschliesslich vom Bindegewebe ableiten, durch an sich unverstaendliche, weil in ihren Causalmomenten nicht darzulegenden Veraenderungen jenes Gewebe an den betreffenden Orten entstanden uns vorstellen, sondern den wesentlichsten Antheil bei seiner Entstehung in den Osteoblasten finden, werden die ersten Anfaenge der Hautskeletbildung (Selachier) mit den hoechst verwickelten Zustaenden des Skeletes der Wirbelthiere aufs innigste verknüpft."

The sentence just quoted, rather typical of the style, defies direct translation, but its sense may perhaps be rendered as follows: By referring the most essential share in the formation of the bony skeleton to the osteoblasts, we are enabled to connect intimately the first beginnings of the dermal skeletal formation (of Selachians) with the highly differentiated condition of the skeleton of the higher Vertebrata. We no longer derive the bony skeleton exclusively from the connective tissue, and we no longer imagine that these tissues arose *in situ*, owing to changes which, as their causes cannot be explained, are by themselves not understandable.

In connection with the above consideration may be mentioned the following important views (p. 152): Although treated as an hypothesis, it appears likely that enamel, dentine, bone substance, are all of ectodermal origin, and the same is probably the case with cartilage. And p. 200: There are reasons for assuming, although not yet absolutely sure, that the whole mass of the placoid scale, including the bone, arises from the ectoderm. Anyhow, all the hard structures, and all their bony formations are derived from the integument. In the higher creatures the bone-forming material is already mixed up with the mesoderm, and our author thinks that the perichondrium has received its bone-forming elements likewise from the integument.[1] Some skleroblasts are certainly identical with osteoblasts, and these latter become eventually bone-corpuscles.

Anyhow, at last, the bugbear of the fundamental difference between tegumentary (so-called secondary, in reality primary) and chondral bone, and that between endo- and ecto-chondral bone, is recanted. Gegenbaur himself had introduced these differences, which have been used for the last thirty years as an all-powerful conjuring formula. He has laid the ghost originally raised by himself; laid perhaps too effectively, as the old terms could easily be made to receive a corrected meaning, in harmony with the old well-meant distinction, if—instead of primary and secondary bones in the old sense—we put primary and secondary elements of the primordial skeleton.

The author (p. 186) is in favour of the connection of the so-called notochord (which he would rather not straightway call a notochord) of Cephalodiscus, Rhabdopleura and Enteropneusta, with that of the Tunicates. The lucid treatment of this important structure may be used as an example of how the conditions prevailing among Invertebrata can be made to throw light upon the more complicated Vertebrata. He argues, namely, as follows:

> "The origin of the chorda from entoderm, from the gut-wall, is phylogenetically not understandable. Such a string of cells must originally have had another function than that of support. It is therefore reasonable to derive the chorda from some gut-diverticulum, such as actually does exist at the very place whence phylogenetically the chorda must have begun; the diverticulum, as well as the chorda arising, moreover, at the spot where ento- and ecto-derm meet. And although such diverticula no longer exist in Tunicates, their ancestors must necessarily have possessed them, because some of the recent Tunicates have a chorda. Moreover, the occurrence of such diverticula in several otherwise divergent forms, as, for instance, Cephalodiscus and Rhabdopleura, indicate a former, more widespread existence."

All this is very satisfactory, but how does it fit in with what we read further on about the brain and the spinal cord? Gegenbaur, namely, has always held that the brain is the older, and the spinal cord the newer formation. Considering the importance of this question for the origin of the Vertebrata from Invertebrata, we turn eagerly to what he has now to say on this point. But instead of receiving comfort, we are led into a maze. See for this the following three statements:

(P. 724.) The phylogenetic value of the ontogenetic mode of origin of the spinal cord, namely, as a further continuation, or budding, from the brain, can be contested. Because such a mode of formation would presuppose a condition in which the spinal cord had preserved its ectodermal position in the ancestors of the Acrania. Only the epichordal nerve-string of the Tunicates represents a sort of early stage of the spinal cord, but in reality it is not yet a spinal cord.

(P. 725.) Exclusively ontogenetic treatment of this question shows that the spinal cord is phyletically produced by a successive budding from the archencephalon. But if this were really so, then the otherwise so well-founded connection with the Tunicates would disappear....

On p. 779 the author holds that the mode of formation of the cord in Petromyzon and in Teleostei, as a solid string with subsequent appearance of a central canal, is more primitive than the early formation of a semi-canal of the medullary plate as observed in all the other Vertebrata.

These three statements are not easily reconciled, and that ontogeny supports the budding mode is an assertion at least surprising. But behold, on p. 718 and p. 719, concerning Tunicates, we are told that:

> "Die Medullarplatte senkt sich in die Tiefe, besonders hinten, wodurch eine Taschenform entsteht. Die nach vorn sich weit oeffnende, hinten geschlossene Tasche erstreckt sich immer weiter nach hinten, indess ihre aeussere Oeffnung sich verengt, und als Neuroporus weiter besteht."

At first sight the reader will think that *nach hinten* is a *lapsus calami*, instead of *nach vorn*, but this is not the case. The sentence also reads as if neuropore and blastopore were the same, but a few lines further on we are categorically informed that certain other features, for instance the neurenteric canal, "are cenogenetic, and have to be passed over." In fact the author translates into growth backwards what ontogeny clearly shows to be the opposite, namely, the closing in of the canal from the blastopore forwards. Consequently the secondary nature of the spinal cord, as a budding from the brain, receives no support from ontogeny, while according to the author himself it can be contested upon phylogenetic grounds!

For the rest we cannot do more than single out a few paragraphs as samples, be they typical illustrations of the masterly treatment of the whole work, or be they falling short of, or running contrary to, our expectations.

The author emphasises the great difference between Sauropsida and Mammalia, and as we read further through the book all the differences are made much of, until the only group worthy of possible ancestral relationship are the Amphibia, notably Anura. This old view has been revived, and it is difficult to say if this unfortunate notion of Amphibian descent "mit Umgehung der

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[1] It is difficult to see how this tegumentary origin of cartilage (for which also see p. 152) can be reconciled with the statement made on p. 241—"An additional argument against the exclusive origin of cartilage from mesoderm is the transformation of chordal into cartilage cells"—even if this so-called chordal cartilage had not been shown by Zykoff and others to be an erroneously interpreted observation.

Sauropsiden" is a conclusion arrived at from structural grounds, or if it in turn is responsible for the acceptance of certain morphological conclusions. One, out of many, is the sanction given to the startling attempt made by his prosector, to derive the hair from something older than Sauropsidan structures, namely, from certain degenerating sense organs of Amphibia. A very able criticism of this hypothesis has, by the way, been given by Keibel in Merkel and Bonnet's periodical. The same idea of relationship underlies the treatment of the skin-glands, the skeleton of the limbs, the homologies of the ear-bones, &c.

The paragraphs dealing with the vertebral column are not a success; the question is left where Klaatsch left it in 1895, and that does not go beyond the cartilaginous fishes; the rest is written on the old futile lines. Nor can much praise be bestowed upon the treatment of the ribs, in which—instead of his old well supported teaching—a compromise has been made adopting the view of the existence of upper and lower ribs, mixing up thereby true ribs and other parts, which, although likewise differentiations of ventral arches, are not, nor ever were ribs. At the same time there is a tenacious pleading for the correctness of his original view. One of the causes of this uncertain state is the wholesale adoption of the results arrived at by one of his assistants.

Perhaps all through the book an easily understood, but nevertheless somewhat undue preference is given to papers published in the *Morphologische Jahrbuch*. Herewith is connected a peculiar treatment of the literature, which shows many deficiencies, more due to design than to accident, because the author says pointedly that he has restricted himself to the most important writings. Anyhow, this may be said in explanation: The MS. seems to have been practically finished several years ago, and little notice has been taken of what has been published since, except the work done by those with whom the author has been more or less in contact, bringing thereby some additions down to even last year's publications.

The genesis of the Chelonian carapace is a most suggestive chapter, but difficult reading, especially since, a page or two further on, the right of existence of an alternative view is conceded, namely, that neural and costal plates may after all be of dermal origin. Dermochelys, in support of the leading view in the text, is pronounced to be a low, most primitive form, but a little further on the mosaic shield of this turtle is confessed to be in a state of retrogression.

The visceral, branchial skeleton of the Cyclostomes (p. 415) is of ectodermal origin. "It must, however, have been derived from the cranial cartilage, for we cannot possibly side with the old teleological notion that this cartilage arises in the ectoderm *in situ* of the future branchial skeleton, and develops itself into the necessary branchial arches. A small interbranchial bit of cartilarge, consisting perhaps of a few cells only, would be of no use, and, moreover, why should such isolated bits of cartilage cells turn up there? But the assumption that the branchial apparatus arises as little processes of the already existing cranium, gives them at once a supporting function." Then follows a beautiful, lucid description of the branchial skeleton of the Gnathostomata, the whole long chapter, from Elasmobranchs to the highest Mammalia, with all the marvellous modifications, ultimately into hyoid, epiglottic and laryngeal apparatus being welded into an harmonious illustration of onward evolution.

On p. 460, in winding up with a fascinating *résumé* concerning the emancipation of the head from the rest of the body, the author takes the opportunity of severely reproaching the embryologists.

"Comparison teaches us that the visceral arches of the Amniota are derived from branchial arches; the descriptive method reveals only the differences, and takes no stock of the fact that these gill-less visceral arches were once gill-bearing arches. This knowledge is the result of the comparative method and of conclusions based upon these facts, conclusions which stand in contradiction to ontogenetic experience. But while this has not prevented any one from acknowledging the homologies of visceral and branchial arches, the same kind of conclusion when applied to the cranium [namely, to its metameric origin] meets with objections."

But was this outburst necessary? Who, having followed the last ten years of extremely active, and chiefly embryological research, does now object to the metameric composition of the head?

Curiously enough, his famous theory of the origin of the limbs and their girdles from visceral arches is only sketched in the very outlines. He devotes but a few very critical remarks to the important support, which his theory has gained from ontogenetic research, and how far ontogeny can be expected to yield results. This is surprising, because nobody, unless he has studied the whole question and the literature attentively, will be convinced by the perusal of the few pages 461-466. We had the right to expect a critical marshalling of the whole apparatus of comparative anatomy, with the numerous points in favour, drawn from the skeletal, muscular, and nervous systems, and refutation of the almost equally numerous mistaken contrary comments. The derivation and evolution of the free limb is, however, done splendidly, although we miss a discussion of the axis of the dactyloid limb. The author himself says, in the preface, that he could not treat everything with equal fulness, but there are not a few points on which we should have liked this very authority's opinion. We look, however, in vain for more than a passing remark under epitrichium, subnotochordal rod, proatlas, pisiform bone, os acetabuli, &c. He holds that there were never more than five fingers "unless a creature can be shown which normally possessed more." All the so-called vestiges of additional fingers and toes are discarded summarily; but might not Kükenthal's discovery of excalation of fingers in the Cetacea have been given at least passing consideration?

More than 100 pages are devoted to the muscular system, making quite a new feature in a text-book, considering how scantily it had been treated hitherto from a general point of view. It is the first successful attempt to bring the mass of accumulated observations into one frame, beginning with the invertebrate conditions as the fundamental starting-point, and then paying especial attention to the muscles of the head, and to the derivation of those of the limb from the truncal system. The nerve-supply is of course taken as the guide, while the almost endless modifications are in many cases followed back to more primitive conditions. It is scarcely necessary to say that Gegenbaur himself initiated this treatment, culminating in the elaborate researches of Fuerbringer and Ruge.

The grouping of the cranial nerves is also quite new, but any attempt to arrange the cranial and cranio-spinal nerves without reference to their origin in the central ganglionic columns, must needs lead to failure; and that is the case here. The metamerism of the cranial nerves implies a problem which is not only morphological but essentially physiological, and here was a chance for the morphologist to join hands with his physiological brother, instead of the usual complaint about the abstaining attitude of the latter, and absolutely ignoring his histological and experimental work.

Whilst discussing the modifications of the visceral arches, concerning the formation and the homologies of the ear ossicles (this vexed question has entered a new phase, far from being at rest), he makes the following remark: "Although none of these cases [certain reptilian modifications] are immediate preparations for the mammalian condition, they nevertheless appear as attempts towards this new modification." This is by no means the only instance of his speaking of attempts, or preparations, precocious and aborted, anyhow unsuccessful in one group, foreshadowing arrangements in others; *e.g.* bipedal gait of Dinosaurs with reference to birds.

The last 127 pages are devoted to the sense organs. After a masterly general introduction follows a most interesting chapter on the organs of the "Hautsinn," taste, ear, eye, nose, each with an invertebrate prolegomenon, full of descriptive detail and all turned into a broadly conceived, well composed and carefully finished picture.

There is no book like this one on comparative anatomy. In broadness of plan, depth of conception, and critical execution it cannot be surpassed. Instead of being a fund of detailed facts, as some of its predecessors, it is a mine of wealth of most suggestive ideas.

If we have in some respects found fault with the book, the explanation suggests itself that the architect and master-builder has too trustingly taken over his material as sound and flawless from the bricklayers, carpenters, and other helpers. But the partial disappointment may also perhaps be a fault of our own, of those who expected too much from their old master, whom they look up to with a gratitude and reverence bordering on veneration.

H. GADOW.

ELEMENTARY QUANTITATIVE ANALYSIS.

An Introduction to Practical Quantitative Analysis. By H. P. Highton, M.A. Pp. 211. (London: Rivingtons, 1898.)

MR. HIGHTON appears to belong to the growing class of teachers of chemistry who believe that the practical work of beginners should be of a quantitative character. Qualitative analysis may, it is true, be made an excellent drill; it attracts most young students, and its practice, if it be properly taught, undoubtedly promotes the formation of orderly habits, develops the powers of observation, and encourages the use of the reasoning faculties. But if it be ill taught, few studies are of less value.

It has been pointed out again and again that when the classes are large and the teachers few, when the available time for study is brief, and if the students come unprovided with a fair elementary knowledge of chemistry qualitative analysis is sadly apt to degenerate into the futile pursuit known as test-tubing. Moreover, this branch of work does not offer a very good selection of clear and simple illustrations of the fundamental laws of chemistry; and it is these, after all, which we especially want to impress upon the minds of those young students who learn chemistry for the sake of its educational effect, and not, at first, in order to become chemists.

These and other similar considerations, as we all know, have led many schoolmasters to postpone qualitative work to a later stage than that at which it was formerly commenced, especially in schools where it is the practice to turn whole classes of young boys into the laboratory, instead of making them listen to lectures of a more or less formal and didactic character in the lecture-room, according to the older practice. It is natural, therefore, that of late years many attempts should have been made to produce a book suited to the needs of the juniors of to-day, as the "Small Roscoe" and "Little Miller" provided for those of earlier generations, in the times when the number of school laboratories might have been counted on the fingers of a single hand. And although nothing seems yet to have been produced which exactly supplies the existing need, several of the new books have been helpful and suggestive.

Mr. Highton's little book certainly takes rank with this latter class. It contains eighty-six carefully selected experiments, which have all been performed by boys in the Rugby Laboratory. These eighty-six experiments cover a fairly wide field, they are clearly described and illustrated by a number of helpful diagrams, they include several exercises in those parts of physics which are of the most direct importance to students of elementary chemistry, and several of them are quite easy; but their character as a whole suggests that they will be found to be more suitable for the senior boys, than for beginners in the lower forms. For the former they ought undoubtedly to be useful, in spite of a certain want of suggestiveness, in the arrangement of the book, which seems likely to impair the educational value of the course, by leading the student to look upon the experiments as mere bits of manipulative and mental gymnastic, and to overlook their relation to the science as a whole. This is a defect, however, which may be remedied by the teacher, by means of verbal discussions of the results obtained, and by rearranging the order of the experiments to suit his methods of teaching.

Whether Mr. Highton's book will soon find a sufficient field of usefulness, we cannot say. We hope it may, but we fear it may not; for, alas! too many teachers are still very much at the mercy of the examiner, and qualitative analysis still rules supreme in many of the leading examinations at which public school boys compete. Doubtless it is very difficult to make fundamental changes in an examination syllabus, and it must be admitted that the Examining Boards would